

January 9, 1951

Dear Dr. Klieneberger-Nobel:

Thank you for your letter of the 20th, with its photographs and encouraging news. Some of the figures look especially provocative, and I admit, with you, the special significance of the longitudinal associations that you sketch. The main problem will be, of course, to correlate these observations with genetic recombination, and in other ways to justify the conclusion that you have actually seen fusion figures.

Until a few days ago, I would have insisted that one should find the same occurrence in cultures of 58-161 or W-1177 taken singly, as in mixtures. This would leave very little room for controls. Some recent, and still fragmentary observations on strain compatibilities now indicate, however, that W-1177 by itself should perhaps show no self-fertilizations, although they should be found as frequently in 58-161 or K-12 by themselves as in mixtures with W-1177. Of course, genetically meaningful results can only ensue in the mixed cultures. Do your observations accord with this prediction-- that is, have you looked for evidences of fusion in 58-161 and W-1177 taken singly, as well as in matings?

These very recent results may lead us to more satisfactory control of the sexual process in this material. W-1177 and certain other strains appear to be sterile, unless combined with the more usual stocks that are self- and inter-fertile. Since an incompatible mating can be made to occur by conducting a compatible mating (controlled by suitable genetic markers) in the same mixed culture, it may be possible to detect some sort of biochemical control of recombination. At least, this is my present working hypothesis.

In a previous letter, you referred in passing to special procedures that might stimulate fusion; here you write only of matings. Have you simply been plating the washed suspensions on minimal agar under the conditions I wrote to you about (except for temperature), or have you found some especially favorable circumstances? If you think your work has gone sufficiently far, I should like to join in a genetic corroboration.

If you can manage it later this year, your visit to this country should create a splendid opportunity to coordinate our viewpoints by work and discussion at close hand. I do hope you will be able to spend some time in this laboratory; at all odds, I await your plans with great interest.

With best wishes for 1952,

Yours sincerely,

Joshua Lederberg